

Columbia University
in the City of New York
[NEW YORK 27, N. Y.]
DEPARTMENT OF ZOOLOGY

12/16/47

Dear Josie,

This long delayed letter is designed to answer two of yours (Oct. 10 Nov. 30) & to give you a little news about your alma mater.

Thanks for the copy of the 679-680 summary. The variation in total population from tube to tube is, as you point out, immense. For that reason it is a poor experiment. The $10.02 \pm 1.99 \times 10^9$ cultures were probably contaminated if I can judge from Lilliams note in the record book. That in one case there were 92 surface colonies present. Excluding these, there is still too much variation compared with what we get with histidineless where the total number is constant not only from tube to tube but from month to month. The reason for this variation, which will be easy to avoid, lies in the conditions of the expt. ~~23 hr.~~ complete medium cultures ~~were~~ were centrifuged & resuspended twice in saline & allowed to stand 8 hrs. before flattening.

The culture was too-old, washing introduced some variation & slugs in salines we know is quite lethal. Next time we'll do better.
But when is next time. I won't try to outline for you our busidinolless data. They become more & more interesting & more & more complex. The complexity, as you will point out, is a function of our ignorance, but I think we are still ~~ver~~ of having a pretty good approximation of an explanation. I hope in about a month to let you look at a manuscript or two. (Probably you will see them on 1950— realistic statement.) However in my optimism I hope to get to Y53 this spring. I won't be at Chicago (Perkins will) so why not come all the way east for a visit & we can talk over this business. Right now—the only way I could explain busidinolless is to write a manuscript or talk with a blackboard & a couple of hours time.

Dave is busy with his corn mutants, now has a lot of biochemicals. He knows about the Sphaerobotheca, stuff for is about ready to go to town on, reconfirmation *in vivo* & *in vitro*. He hopes to be finished this

summer. If you need, or know someone who needs, a first-rate man, keep Dave on mind.

Kim is now getting in stride. I think the most interesting thing he's studying is an adaptation of the ~~other~~ technique to determine the weights of the same mycelial clot at different stages of growth. As you know, he has plenty of imagination, ingenuity. I'm trying to hammer home just a little discipline. I seem always to have gifted problem children, don't I? I like them.

Sheng is very busy with ~~so many~~ (alas, no) courses. He has, however, spent some time on the asparagine adaptation in K12. He cannot confirm what I think you told me i.e. that C₄ acids will substitute for asparagine & aspartic acid. Do you plan to follow through on this? I think Sheng is interested in doing so. There is a peculiar inheritance of the adaptation.

Kim, by the way, is another father, as of Sunday night—a girl named

June. All are well. Carl Clark, as an
additional by the way, gets married
this Saturday.

Mrs. Lencliter Berg, & Peg Lieber are learning Neurospora techniques in '20 of 219. As a small problem they have studied the segregation of nuclei into branches in a heterokaryon. Segregation does occur. Tom Nelson just handed in a report on the respiratory metabolism of Neurospora consisting of work he did in 219 last year. He's trying with the idea of following through on that. There's a Brighto Industrial Corp.

So you see there is the floor is burning up.
We've nothing spectacular like Evelyn's
salt mutations nor Gus Doermann's ~~sofa~~
~~arithmetic multiplication~~
~~in linear, but location~~ of phage but we're
just beginning. Lili is getting mighty
pleased with our physical setup which
is improving. In a year or two we'll
hit a jack-pot. Just this month have I really
gotten back in the lab. The administration
trap, departmental but mostly College,
has risen to an all time high. I'm getting
very fed up for its only cry about strength.

* not intelligence that I get any time for my own research. And as you know I get a very large kick out of being in a clean experiment. I don't like to talk, especially about "high" policy like whether the pre-engineers clock to should follow them into Engineering school or should remain in the Deans office. So much for that as I'll love you to death.

Returning to your letters Rubin has contacted me but as yet we have not gotten together. Glad to hear that your facilities are developing favorably & that you're getting things done. You have, by the way, finally converted Beadle to your cause. He still moans thru the utp every once in a while. Did you know that Mitchell found that Daves nicotinic precursor is OH antranilic acid? Delbrück is apparently organizing the mathematicians & theoretical physicists around Biology & is considered a real big asset. I'm sure he is.

What's the story on the microfilm of the Stanford theses? Is the next

move yours or mine? Also are there
any really promising young Crockerists
coming thru at Wisconsin?

This letter has been interrupted by us
interrupting the determination of growth
curves of shaken cultures of $\text{L}^{\text{T}}\text{S}^{\text{I}}$ +
that's too damn bad so ~~so~~ . . .

Regards to Esther, Snell + any
one else would deserve + appreciate them

Sincerely,

Frances